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A replication study of a newspaper campaign in Uganda

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Fighting corruption *does* improve schooling: a replication study of a newspaper campaign in Uganda

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Abstract

In the mid-1990s, Ugandan schools received only one-fifth of government capitation grants for primary education. Reinikka and Svensson (2005) show that a grassroots newspaper campaign substantially reduced grant capture and subsequently improved educational outcomes. This study replicates Reinikka and Svensson's results and examines their robustness. Recreating the same dataset and applying the same methods, our pure replication confirms that the newspaper campaign reduced the capture of capitation grants and improved enrolment. However, the latter result that the additional funding due to the newspaper campaign increased enrolment is robust only if we rely on a more accurate enrolment measure introduced by Reinikka and Svensson (2011). In contrast, we cannot support the authors' conclusion that lower capture due to the newspaper campaign improves learning outcomes. Finally, we provide tentative evidence that the newspaper campaign allowed for a fairer allocation of teachers across schools, a result absent in the original papers.

Keywords: replication, Uganda, corruption, public service delivery, primary education

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Abbreviations and acronyms

DEO	District Education Office
LC	Local council
MEA	Measurement and Estimation Analysis
PTA	Parent-teacher association
PLE	Primary Leaving Exam
PETS	Public expenditure tracking survey
SMC	School Management Committee
TCA	Theory of Change Analysis
Ush	Ugandan shilling
UPE	Universal Primary Education

1. Introduction

The provision of education in developing countries is typically the purview of the government. But the effectiveness of this service provision is another issue entirely. A programme implemented by a government may have vastly different effectiveness when compared to the same programme as implemented by a non-governmental organisation (NGO). For example, Bold *et al.* (2013) investigate the impact of a successful scheme to increase educational attainment via the provision of contract teachers when it is implemented by both the Kenyan government and an international NGO. They find that the positive impact of the contract teacher programme only arose under NGO implementation, while the government-administered programme produced no discernible effects.

If government service provision is indeed less effective, it is crucial to understand why. Bottlenecks and leakages in service delivery chains may complicate scale-up to the country level. In many cases, resource capture plagues public service delivery. After relying on a public expenditure tracking survey (PETS) in Uganda, Reinikka and Svensson (2004) reported the now infamous statistic that, in the mid-1990s, only about one-fifth of government capitation grants[1] for primary education actually made it to schools (p. 687). The rest found its way into the pockets of local officials. Reinikka and Svensson (2004) suggest that the bulk of evidence from other countries in Sub-Saharan Africa points to similar issues.

To counter such blatant rent-seeking, the Ugandan government introduced a newspaper campaign at the end of 1997. Instead of tackling corruption from the top, the campaign took a grassroots approach: national newspapers published information on the amount of capitation grants disbursed by the central government to school districts, as well as on the timing of this disbursement. By providing citizens with better access to information, the Ugandan government hoped to curb capture of the capitation grants by local officials.

Reinikka and Svensson (RS 2005) examine the impacts of this campaign. Their contributions are two-fold. First, relying on a difference-in-differences analysis, RS (2005) evaluate how the anti-corruption newspaper campaign improved the receipt of capitation grants targeted for primary schools. Combining the difference-in-differences analysis with an instrumental variables approach, the authors then examine how this increase in funding due to the newspaper campaign influenced educational outcomes. Indeed, as emphasised in ODI (2006), capitation grants served to finance instructional materials (50%), co-curricular activities such as sports and clubs (30%), school management such as school maintenance and payment for water, electricity, etc. (15%) as well as school administration (5%). It is therefore reasonable to surmise that more funding available for these areas would increase the student body as well as students' educational achievements (although the positive impact of more funding on test scores may be at least partly counterbalanced by the negative impact of larger class sizes and/or an influx of lower-achieving students).

RS (2005) find that the share of capitation grants reaching the school skyrocketed due to the newspaper campaign. Moreover, ‘the reduction in capture had a positive effect on enrolment and student learning’ (see RS [2005]'s abstract). Given that RS (2005) intended to report preliminary results, the authors submit their findings to a wider range of robustness checks in Reinikka and Svensson (2011) and arrive at the same conclusions.

[1] The name ‘capitation grants’ stems from the fact that these grants are computed based on the student body.

We pursue two objectives in this replication report. First, we conduct a pure replication of RS (2005)'s results. Widely cited in the development literature as evidence on the success of transparency and accountability reform, RS (2005) indeed constitutes an important study for replication. Focusing on the governance of social services on a country-wide scale, they are able to detail a specific causal chain from the information campaign to capitation grant receipt to educational outcomes. Their findings hold substantial implications for the improvement of public service delivery in developing countries. Moreover, RS (2005 and 2011) contribute to understanding the policies which promote not only attendance but also learning, a major challenge for development institutions nowadays (see Kremer *et al.* 2013).

Our second goal is to provide additional analysis. This analysis first consists of a Measurement and Estimation Analysis (MEA) which investigates the sensitivity of RS (2005)'s findings beyond the checks originally performed. Notably, we 'update' RS (2005) with the authors' later work from RS (2011). We also conduct a Theory of Change Analysis (TCA) which builds on the original analysis by investigating other educational outcomes likely to be impacted by the newspaper campaign, notably the number of teachers. This has potentially important implications for the impact of the campaign on enrolment and learning outcomes.

Meanwhile, recognising the potential for contention surrounding replication studies, we do our best to address critiques in the ongoing discussion in development community. (See, for example, discussions on the World Bank's Development Impact blog.[2]) We tried to keep the pure replication section of this report as clear as possible so that readers are able to fully understand the original study without reading it. We also took care of avoiding p-hacking by raising the threshold under which a result can be deemed as statistically significant from $p = 0.10$ to $p = 0.15$. Additionally, we adhere to our replication plan and test the robustness of the original results in what we believe to be the most straightforward ways. Finally, results from the additional analysis are original. Our robustness checks are absent from RS (2005 and 2011) as well as from their working paper last updated in 2006 on which RS (2005 and 2011) heavily relied.

Our findings first confirm that the newspaper campaign reduced capture of capitation grants. Moreover, we can endorse the conclusion that more funding due to the newspaper campaign increases enrolment, although this result emerges only if we rely on a more accurate enrolment measure introduced in RS (2011). It fails to hold when we replicate RS (2005). In contrast, we cannot support RS (2005 and 2011)'s assertion that lower capture due to the newspaper campaign improves learning outcomes. Finally, we try to better illuminate the theory of change behind these results. In doing so, we document an additional positive impact of the newspaper campaign. Our results suggest that this campaign allowed for a fairer allocation of teachers across schools, a result absent from the original papers.

The paper proceeds as follows: Section 2 develops the motivation behind this replication. In Section 3, we conduct the pure replication. We perform the additional analysis in Section 4. Section 5 discusses our results. We emphasise the limitations of this replication report in Section 6. Finally, Section 7 summarises our findings and their policy implications.

[2] http://blogs.worldbank.org/impactevaluations/

2. Motivation

Since its publication, RS (2005) has had major implications for the improvement of public service delivery in developing countries as well as for understanding the policies which promote primary school enrolment and attainment. RS (2011) report how similar newspaper campaigns were implemented in Kenya and Tanzania. In this context, it is critical to confirm that RS (2005)'s results can be easily replicated and are robust to straightforward checks in an MEA.

First, relying on a difference-in-differences analysis, the authors estimate whether an anti-corruption newspaper campaign improves the receipt of capitation grants by schools. More precisely, they regress the change in the share of capitation grants reaching the school between 1995 and 2001 on a school's distance to a newspaper outlet in 1997. The lower this distance, they surmise, the higher the school's exposure to the newspaper campaign and therefore the higher the share of capitation grants received. However, to ensure that the effect of distance to a newspaper outlet on grant capture indeed measures the impact of the newspaper campaign, one must guarantee that this impact does not exist during the pre-campaign period. Such an analysis is absent from RS (2005). While it is incorporated into RS (2011), the authors rely on a sample of schools for the pre-campaign period in which some schools are surveyed only once instead of a balanced panel. This procedure can generate substantial bias. For those schools surveyed only once, it is indeed not possible to control for school fixed effects (i.e. for time invariant characteristics at the school level). Ensuring that the results for the pre-campaign period hold with balanced panel data therefore constitutes an important element of the MEA. Moreover, we feel that RS (2011) could control more completely for competing channels through which distance to a newspaper outlet may impact the share of capitation grants reaching school starting from 1997. This concern stems from the UPE (Universal Primary Education) reform which was nearly concomitant to the newspaper campaign. We therefore use this replication to include a wider set of tests with the goal of ruling out the possibility that the UPE reform was driving RS (2005)'s results on grant capture and schooling outcomes.

Similar concerns may arise regarding the authors' second contribution. Does the newspaper campaign impact enrolment and learning outcomes only through more funding? Besides an incomplete accounting of omitted variables which could drive their results, it could be the case that students, notably the highest achievers, enroll in schools with more funding with in fact no impact of a reduction in grant capture on enrolment and academic performance at the aggregate level. RS (2005) address sorting via an alternative estimation strategy (with a different instrument), while RS (2011) do not thoroughly address sorting. We aim to complement this strategy with a more straightforward approach which simply repeats their identification strategy at the district level instead of the individual level.

Though the pure replication and MEA are evidently the key components of our replication, we also try to shed more light on the mechanisms behind the results with a TCA. Our original replication plan proposed incorporating additional educational indicators, chiefly with alternative datasets. However, this plan was not feasible due to geographic incompatibility. Therefore, we simply limit our TCA to the key educational information in the original data: teacher supply and additional funding (teacher salaries and parent-teacher association charges). Our results suggest that the newspaper campaign allowed for a fairer allocation of teachers across schools.

3. Pure replication

We proceed in three steps. We first present RS (2005)'s identification strategy. We then describe our dataset construction. In the last section, we replicate RS (2005)'s results.

3.1 RS (2005)'s identification strategy

RS (2005) rely on an instrumental variables approach. The first-stage estimates the impact of the newspaper campaign on the share of capitation grants reaching schools. The second-stage estimates the impact of the share of capitation grants reaching school due to the newspaper campaign on educational outcomes (enrolment and learning outcomes[3]).

To estimate the impact of the newspaper campaign on the share of capitation grants reaching schools (first-stage), RS (2005) rely on a difference-in-differences analysis. This approach is based on the premise that the probability for a school to be 'treated' by the newspaper campaign negatively depends on the school's distance to a newspaper outlet in 1997, i.e. just before the launch of the newspaper campaign which occurred towards the end of 1997.

The first-stage of RS (2005)'s instrumental variables approach is therefore given by Equation (1):

$$s_{it} = \alpha_{0i} + \alpha_1 year_{1997} + \alpha_2 (distance_i \times year_{1997}) + u_{it},$$

for $t = \{1995, 2001\}$. The dependent variable s_{it} denotes the share of capitation grants reaching school i at date t. Coefficient α_{0i} is a school-specific fixed effect which captures the effect of all time invariant characteristics at the school level. It notably includes the effect of $distance_i$ which represents the distance of school i to a newspaper outlet in 1997.[4] Variable $year_{1997}$ is a binary variable which takes the value 1 after 1997 (hence for year 2001) and the value 0 otherwise (hence for year 1995). Finally, the error term u_{it} captures the idiosyncratic shocks that affect the share of capitation grants reaching school i at date t.

In Equation (1), the difference-in-difference coefficient is estimated by α_2, provided that the control group and the treatment group follow parallel trends over time prior to the newspaper campaign. But for α_2 to capture the treatment effect, one must also ensure that no omitted variables bias is at work after the campaign. This implies showing that the distance of school i to a newspaper outlet in 1997 remains statistically significant when one controls for correlates of distance to a newspaper outlet which also likely influence the share of capitation grants reaching school *after* the launch of the campaign.

The second-stage of RS (2005)'s is then given by Equation (2):

$$y_{it} = \beta_{0i} + \beta_1 year_{1997} + \beta_2 \widehat{s_{it}} + v_{it}$$

[3] The latter dimension is explicitly highlighted in RS (2011) and, although presented as preliminary in RS (2005), it is highlighted in their abstract: 'the reduction in capture had a positive effect on enrolment *and student learning*.'

[4] RS (2006) discuss issues related to using distance as an instrument on page 12, including the potential for unobserved school characteristics to be correlated with newspaper access and a school's ability to obtain capitation grants as well as measurement error introduced via parental and community action.

for $t = \{1995, 2001\}$. The dependent variable y_{it} denotes an enrolment or learning outcome, β_{0i} is a school-specific fixed effect and $\widehat{s_{it}}$ is the predicted value of s_{it} derived from Equation (1). Variable $year_{1997}$ is defined as in Equation (1). The error term v_{it} captures the idiosyncratic shocks that affect educational outcomes in school i at date t. The impact of the share of capitation grants reaching a school due to the newspaper campaign on educational outcomes is captured by coefficient β_2, provided that there is no omitted variables bias. This implies that the distance of school i to a newspaper outlet in 1997 impacts y_{it} *only* through the newspaper campaign and thus provides support for the exclusion restriction.

To eliminate school fixed effects, RS (2005) difference Equations (1) and (2). In other words, the authors compute in the first-stage the OLS estimates of Equation (3):

$$\Delta s_{it} = s_{i,2001} - s_{i,1995} = \gamma_0 + \gamma_i distance_i + \Delta u_i \, .$$

Similarly, they compute in the second-stage the OLS estimates of Equation (4):

$$\Delta y_i = y_{i,2001} - y_{i,1995} = \delta_0 + \delta_1 \widehat{\Delta s_{it}} + \Delta v_i,$$

where $\widehat{\Delta s_{it}}$ is the predicted value of Δs_{it} derived from Equation (3).

3.2 Dataset construction

RS (2005) examine the impact of an increase in the share of capitation grants reaching school due to the newspaper campaign on total enrolment. This information stems from Uganda's Public Expenditure Tracking Survey (PETS) conducted among head teachers in 1996 and 2002, respectively. The original 1996 sample consisted of 250 schools, randomly drawn from 18 districts. The number of schools which were surveyed both in 1996 and in 2002 amounts to 250-32=218.[5] RS (2005) rely on this group of schools to generate their (first-difference) OLS estimates of Equation (3) and Equation (4). Note that an additional 170 schools from 9 of the original 18 districts were surveyed in 2002. Therefore, there are 218+170=388 schools in the 2002 PETS dataset.

The share of capitation grants reaching the school in year t ($t = \{1995, 2001\}$) is defined as the total amount intended for year t and received by a school at year t (or at the beginning of year $t + 1$), out of the capitation grant that the central government is supposed to have disbursed for this school at year t. A school's capitation grant is based on the number of students in grades P1−P3 and P4–P7. In 1995, the grant formula allocated 2,500 Ugandan shillings (Ush) a year for each student in grades P1−P3 and 4,000 Ush for each student in grades P4–P7. In 2001, due to the introduction of UPE at the beginning of year 1997, the amounts had doubled: 5,000 Ush for grades P1−P3 and 8,100 Ush for grades P4–P7.[6]

[5] Unfortunately, not all of the original 250 schools could be resurveyed in 2002 because of security concerns. Two districts (Moroto and Bundibugyo) were dropped, reducing the sample by 20 schools. Furthermore, one district (Gulu) experienced a major insurgency during the data collection phase. This led to dropping an additional 11 schools. Finally, one school in the original sample had closed in 2002.

[6] As stressed by Hubbard (2007): 'Compulsory parental contributions had previously been a mainstay of school funding, but were abolished by UPE. Students whose parents could not afford the fees now enrolled in school. Some schools still managed to extract parental contributions, but a household expenditure survey suggested that this had fallen from more than half, to a quarter of primary school funding in 2000. To compensate schools for the loss, the school supply capitation grant was rebranded as the UPE Capitation Grant, and its nominal value per student was doubled.'

We compute the total amount that was intended for 2001 and received by a school in 2001 (or at the beginning of year 2002) by relying on the following three questions from the 2002 PETS:

- Q87: 'Did you receive a UPE cheque (or UPE cash) from district in: (a) January 2001; (b) February 2001; (c) March 2001; (d) April 2001; (...); (m) January 2002; (n) February (2002); (o) March 2002?'
- Q88: 'If yes, how much was the cheque received [for each of these months] in Ush?'
- Q89: 'If yes, what months should this installment cover?'

Regarding question Q89, the enumerator was requested to code the head teacher's answer as 'xxyy', where 'xx' is the month and 'yy' is the year. For example, an installment covering February 2001 was to be coded as '0201'. We start by considering as 'missing' answers to question Q89 which concern a year other than 2001 (i.e. year 2000 or 2002), as well as answers that are not reported in the correct month-year (xxyy) format. This process leads us to set the following 15 responses to question Q89 as 'missing': 102, 202, 302, 402, 502, 602, 900, 1000, 1007, 1020, 1100, 1120, 1200, 1220 and 3012. Note that RS (2005) consider as 'missing' only 8 of these 15 types (namely 900, 1000, 1100, 1200, 102, 202, 302 and 402). However, this omission is not consequential for the results: Based on question Q88, we compute the amount received by the school for each month between January 2001 and March 2002 and intended for 2001. When the school does not specify the month for which a particular grant is intended, we treat these cases as 'missing.' All the instances in which the month(s) of instalment is (are) specified include months in the year 2001 (sometimes combined with months in year 2000 and/or year 2002 beyond March 2002, or with information not reported in the correct month-year [xxyy] format). Therefore, failing to consider as missing a handful of responses to question Q89 which are in the incorrect format, as RS (2005) do, has no bearing on the total amount that was intended for 2001 and received by a school in 2001 (or at the beginning of the year 2002).[7]

We compute the capitation grant that the central government is supposed to have disbursed for a school in 2001 based on the following grant formula: 5,000 Ugandan shillings (Ush) a year for each student in grades P1−P3 and 8,100 Ush for each student in grades P4−P7. However, discussion with the authors informed us that they could not determine whether the capitation grant would be calculated from the student body at the beginning, middle or end of the year or if it was adjusted throughout the year. The 2002 PETS provides two sets of questions about the student body: the total number of students in each group (P1−P3 and P4−P7) at the beginning of 2001 (questions Q12 and Q13) and the total number of students in each group at the end of 2001 (questions Q17 and Q18). The authors compute two capitation grants: one based on student body at the beginning of 2001 (i.e. Q12 x 5000 + Q13 x $8100), the other based on student body at the end of 2001 (i.e. Q17 x 5000 + Q18 x $8100). As is apparent in their .do file (although not specified in their paper), RS (2005) retain the maximum of these two values. Their approach therefore provides a lower bound for the share of capitation grants reaching the school in 2001.

[7] It is possible, however, that some schools reported a single monthly amount which was not intended exclusively for 2001. To be sure, it would be more appropriate to focus on schools which received monthly amounts only intended for 2001. But this is in fact not an option since this would amount to removing 122 out of the 218 schools in the panel sample. Indeed, 122 of these schools received an amount not fully intended for 2001 in at least one of the 15 months covered by question Q88.

Indeed, we compute the share of capitation grants reaching a school in 2001 as the ratio between the total amount that was intended for 2001 and received by a school in 2001 (or at the beginning of 2002), and the capitation grant that the central government is supposed to have disbursed for this school in 2001. Therefore, the higher the student body, and hence the capitation grant supposed to have been disbursed, the lower the share of capitation grants reaching the school. Note that RS (2006) mention that some funds were withheld if districts did not submit the required quarterly documentation. RS (2005) adjust for this feature by scaling down a school's entitlement by the share of funds actually disbursed by the central government. We follow the same procedure.

Correspondence with the authors informed us that they proceeded in a similar manner to construct the share of capitation grants reaching a school in 1995. But we cannot replicate the construction of this variable as the authors did not keep readily accessible records of the raw data for most of the questions from the 1996 PETS which notably included the equivalent of questions Q87 to Q89 of the 2002 PETS and the body of students enrolled in P1−P3 and in P4−P7. Regarding the student body, only one measure was collected (it is not clear whether the head teacher reported the number of students at the beginning, middle or end of the year or if it was adjusted throughout the year). The 1996 PETS therefore differs from the 2002 PETS in which two measures, one at the beginning of 2001 and the other at the end of 2001, were collected. Recall that RS (2005) focus on the maximum of these two values to compute the capitation grant that the central government is supposed to have disbursed for a school in 2001, thereby providing a lower bound for the share of capitation grants reaching the school in 2001. This implies that $|\Delta s_{it}|$ – the absolute value of the difference in the share of capitation grants reaching school in 2001 and 1996 – is, if anything, an underestimate of the absolute value of this true difference. We endorse the authors' approach since it limits the possibility to measure a statistically significant impact (i) of distance to a newspaper outlet in 1997 in Equation (3); (ii) of an increase in the share of capitation grants reaching a school due to the newspaper campaign in Equation (4). Indeed, data analysis shows that the variation in Δs_{it} is lower when it is based on the lower bound of the share of capitation grants in 2001 rather than on the higher bound of this share.

A school's total enrolment is the main educational outcome assessed by RS (2005). More precisely, Δy_i, the dependent variable in Equation (4), is the change in total enrolment between 1995 and 2001. As discussed, RS (2005) choose to retain the maximum of the two measures of total enrolment in 2001 which are provided by the 2002 PETS. We check that RS (2005)'s results are robust to relying on the minimum of these two values in the pure replication section.

Distance to a newspaper outlet in 1997 is proxied by distance to a newspaper outlet in 2001. This variable comes from question Q29 of the 2002 PETS which asks the head teacher to report the ‘distance to the nearest place to buy a newspaper (in km).’ As specified in RS (2006), RS (2005) use the standard natural logarithm of one plus the distance in kilometer to the nearest newspaper outlet.

3.3 Replication of RS (2005)'s results

3.3.1 Table 1

Table 1 of RS (2005) reports summary statistics on the share of capitation grants reaching school in 1995 and 2001 from the original paper (Panel A) and from the replication (Panel B). RS (2005) focus on the 218 schools that were surveyed in both 1996 and 2002. The figures are exactly the same in Panel A and in Panel B. They reveal a substantial increase in the share of capitation grants reaching schools between 1995 and 2001.

Table 1: Summary statistics on the share of capitation grants reaching school in 1995 and in 2001 (in percent)

	Mean	Median	St. dev.	Obs.
Panel A: Original				
1995	23.9	0	35.1	229
2001	81.8	82.3	24.6	217
Panel B: Pure replication				
1995	23.9	0	35.1	229
2001	81.8	82.3	24.6	217

3.3.2 Table 2

In Table 2, RS (2005) seek to better document the missing link in Equation (3), i.e. that a lower distance to a newspaper outlet in 1997 increases the share of capitation grants reaching a school *by improving the head teacher's knowledge about the amount and timing of release of the capitation grant entitled by the central government to his/her school*. Eliciting this missing link implies computing, in a first-stage, the first-difference OLS estimates of Equation (5):

$$\Delta know_i = know_{i,2001} - know_{i,1995} = \lambda_0 + \lambda_1 distance_i + \Delta w_{1i},$$

where $know_{i,t}$ is the knowledge about the capitation grant of the head teacher in school i at date t. Eliciting the missing link in Equation (3) then implies computing, in a second-stage, the first-difference OLS estimates of Equation (6):

$$\Delta s_i = s_{i,2001} - s_{i,1995} = \rho_0 + \rho_1 \Delta\widehat{know}_i + \Delta w_{2i},$$

where $\Delta\widehat{know}_i$ is the predicted value of $\Delta know_i$ derived from Equation (5).

Yet, the head teacher's knowledge about the capitation grant is available only in 2001, so the first-difference approach is not an option. A cross-sectional approach is an alternative which involves estimating Equations (5) and (6) based on the 2002 PETS dataset:

$$know_{i,2001} = \phi_0 + \phi_1 distance_i + w_{3i,2001},$$

$$s_{i,2001} = \xi_0 + \xi_1 \widehat{know_{i,2001}} + w_{4i,2001}.$$

To be sure, contrary to the first-difference approach, the cross-sectional approach will not neutralise school fixed effects which are embedded in the error terms $w_{3i,2001}$ and $w_{4i,2001}$ of

Equations (7) and (8), respectively. This cross-sectional approach therefore falls prey to an omitted variables bias, both in the first- and second-stages. In the first-stage, it could be that the distance of school i to a newspaper outlet in 1997 is correlated to other school characteristics which themselves determine the head teacher's knowledge about the capitation grant. In the second-stage, it could be that the distance of school i to a newspaper outlet in 1997 has an impact on the share of capitation grants reaching the school through channels other than the head teacher's knowledge. In absence of further controls in Equations (7) and (8), the results stemming from the cross-sectional approach must therefore be interpreted with caution.

RS (2005) do not rely on the cross-sectional approach but on a mix between the first-difference and the cross-sectional approach. The authors estimate Equation (7) in their first-stage and Equation (6) in their second-stage where $\Delta\widehat{know}_i$ is replaced by $know_{i,2001}$. Panel A of Table 2 reports the results from the original paper. Note that we are able to exactly replicate these results, which we show in Panel B, labelled 'Pure replication.'[8] However, since it is standard to maintain the same sample and specification in both stages of the 2-SLS, we present in Panel C, called 'Modified replication,' the OLS estimates of the cross-sectional approach (i.e. of Equations (7) and (8)). We get these results thanks to the standard *ivregress 2sls* command in Stata.[9] We clarified the decision to estimate the two-stages with the same sample with the authors, who were supportive.

[8] RS (2005) rely on two questions from the 2002 PETS to determine the head teacher's knowledge about the capitation grant. The first is question Q58: 'Do you know the school's entitlement of UPE capitation grant per student in 2001?' which is asked separately for P1−P3 and P4−P7. If head teachers are correct about both groups, we code this variable as one (and zero otherwise). Further communication with the authors indicated that they allowed for an error of $\pm 5\%$ in the head teachers' responses. We proceed in the same manner. The second question about head teacher's knowledge is question Q63: 'Do you know when the district receives funds for UPE from the Ministry of Finance?' Again, we code this variable as one if the head teacher knows when the district receives funds (and zero otherwise). The final head teacher knowledge variable is a sum of the two previous binary variables (head teacher knowledge of entitlement and head teacher knowledge of timing), yielding a value of 0, 1, or 2. Note that the authors also include mean consumption at the district level as a control, based on national household survey data. Following their approach, we control for this variable in *all* the regressions presented in this replication report.

[9] Results are the same when we use *ivreg2* instead.

Table 2: Linking distance, head teacher's knowledge, and share of capitation grants reaching school

	Dep. var.	
	HT's knowledge in 2001	Δs
	(1)	(2)
Panel A: Original		
Distance to a newspaper outlet	-0.10***	
	(0.03)	
HT's knowledge in 2001 (inst.)		65.88***
		(23.50)
		[25.47]
Number of schools	388	199
Panel B: Pure replication		
Distance to a newspaper outlet	-0.10***	
	(0.03)	
HT's knowledge in 2001 (inst.)		65.88***
		(23.50)
		[25.47]
Number of schools	388	199
Panel C: Modified replication		
Distance to a newspaper outlet	-0.12***	
	(0.03)	
HT's knowledge in 2001 (inst.)		41.54***
		(17.79)
F-statistic	13.35	
Number of schools	375	375

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Consumption at the district level is included as a control. OLS standard errors are in parentheses, and bootstrapped standard errors are in brackets. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

Despite the change in procedure, the first-stage coefficient (column 1 of Panel C) associated to distance to newspaper outlet is very similar to the original first-stage results. It is negative and statistically significant at the 99% confidence level, revealing that distance to a newspaper outlet in 1997 is negatively correlated with a head teacher's knowledge about the capitation grant. Moreover, although the magnitude of the point estimate associated to head teacher's knowledge in the second-stage decreases to 41.54 (column 2 of Panel C), it remains positive and statistically significant (at the 95% confidence level). Thus, the share of capitation grants reaching school is positively correlated with the head teacher's knowledge about the capitation grant. It is important to stress that these results hold if standard errors are robust. All in all, RS (2005)'s results reported in Panel A of Table 2 are fully robust to a pure replication using the same data and methods (Panel B) and a more standard approach (Panel C). They are consistent with the premise that a lower distance to a newspaper outlet in 1997 increases the share of capitation grants reaching school *by improving the head teacher's knowledge about capitation grants.*

3.3.3 Table 3

In Table 3, RS (2005) present OLS estimates of Equations (3) and (4) where Δy_i stands for the change in total enrolment between 1995 and 2001. More precisely, in column 1, the authors simply regress Δy_ion a constant to determine the sign of this change. In columns 2 and 3, they report the OLS estimates of Equations (3) and (4). In column 4, they estimate the reduced form of Equations (3) and (4) by regressing the change in enrolment between 1995 and 2001 on distance to a newspaper outlet in 1997. In column 5, they estimate this reduced form for the 1991–1995 period, rather than for 1995–2001. Doing so serves as a falsification test of the exclusion restriction. Indeed, if distance to a newspaper outlet only captures exposure to the newspaper campaign, then the coefficient of distance to a newspaper outlet should be negative and statistically significant in column 4 (since year 1997 is included in the 1995–2001 period) but not statistically significant in column 5 which covers only the pre-campaign period.

RS (2005) discard those schools which experienced a reduction in the student body due to 'idiosyncratic shocks,' noted in footnote 9 on page 264. Yet we are uncomfortable with the decision to focus only on schools which experienced an increase in total enrolment between 1995 and 2001 in the first four columns. If these shocks are truly idiosyncratic, they should not be systematically correlated with distance to a newspaper outlet and keeping them in the analysis should be inconsequential. We therefore run the replication of columns 1 through 4 by reintegrating the 10 schools that were dropped by RS (2005) for reporting a decrease in their total enrolment. We do, however, follow RS (2005)'s exact approach for column 5 where they focus on the sample of schools such that distance to a newspaper outlet as well as change in total enrolment and consumption between 1991 and 1995 is non-missing.[10]

Panel A of Table 3 reports the results from the original paper, Panel B reports the results from our pure replication using the same data and methods as the original authors, and Panel C reports our replication when keeping the 10 schools in question. Column 1 reveals a statistically significant increase in school total enrolment in all panels. Moreover, OLS estimates of Equation (3) confirm, in all panels, the negative and statistically significant correlation between distance to a newspaper outlet and difference in the share of capitation grants reaching schools between 1995 and 2001 (column 2 of Table 3).

[10] The 1996 PETS reports total school enrolment in 1991.

Table 3: Impact of the newspaper campaign on school total enrolment

	Dep. var.				
	Δ tot. enroll. 95-01	Δ s 95-01	Δ tot. enroll. 95-01	Δ tot. enroll. 95-01	Δ tot. enroll. 91-95
	(1)	(2)	(3)	(4)	(5)
Panel A: Original					
Constant	450.47***	74.13***	29.66	574.18***	68.4**
	(20.34)	(6.77)	(286.20)	(49.31)	(33.6)
Distance to a newspaper outlet		-5.74**		-37.74**	-4.7
		(2.45)		(17.94)	(12.1)
$\widehat{\Delta s}$			7.55[a]		
			(4.63)		
Number of schools	202	188	188	202	153
Panel B: Pure replication					
Constant	450.47***	74.13***	29.66	574.18***	100.01**
	(20.34)	(6.77)	(286.20)	(49.31)	(33.26)
Distance to a newspaper outlet		-5.74**		-37.74**	-4.45
		(2.45)		(17.94)	(11.60)
$\widehat{\Delta s}$			7.55[a]		
			(4.63)		
Number of schools	202	188	188	202	153
Panel C: Modified replication					
Constant	418.40***	75.76***	362.42*	522.48***	100.01**
	(21.99)	(6.79)	(186.89)	(53.36)	(33.26)
Distance to a newspaper outlet		-6.83***		-12.13	-4.45
		(2.43)		(19.15)	(11.60)
$\widehat{\Delta s}$			2.22		
			(3.05)		
F-statistic		7.93			
Number of schools	212	198	198	212	153

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS standard errors are in parentheses, and bootstrapped standard errors are in brackets. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

However, while in the original paper column 3 reveals a weakly positive and statistically significant correlation (with the p-value equal to 0.103) between the increase in the share of capitation grants due to the newspaper campaign and the increase in school total enrolment between 1995 and 2001, this correlation loses statistical significance in Panel C.[11] Consequently, the negative coefficient of distance to a newspaper outlet in the reduced form equation in column 4 is also not statistically significant in Panel C, while it is statistically significant in Panels A and B. Finally, our pure replication results for the falsification test reported in column 5 coincide exactly with those stemming from the authors' original .do file

[11] It also loses significance if, instead of integrating the 10 schools which experienced a decrease in their total enrolment, we only integrate the two which experienced a modest decrease (less than 5% of their total enrolment in 1995).

(see Panel B). Note that results reported in Panel C do not change if standard errors are robust. They are also not affected if, instead of keeping the maximum value of total enrolment at the beginning and at the end of 2001 to proxy for total enrolment in 2001, we retain the minimum of these two values.[12] A more straightforward approach than the one used by RS (2005) for columns 1 to 4 of Table 3 therefore supports their claim that the newspaper campaign reduces capture of public funds. However, it does not confirm that 'the reduction in capture [induced by the newspaper campaign] had a positive effect on enrolment' (see RS [2005]'s abstract).

Finally, though it is not consequential since the coefficient of 'Distance to newspaper outlet' is not statistically significant, we note that the estimates from Panel A and Panel B do not coincide in column 5. In Panel A, taken from the published version of the paper, the OLS estimate of the constant and its standard deviation are equal to 68.4** and 33.6, respectively (against 100.01*** and 33.26 in column 5 of Panel B). Moreover, the OLS estimate of the coefficient of 'Distance to newspaper outlet' and its standard deviation are equal to -4.7 and 12.1, respectively (against -4.45 and 11.60 in column 5 of Panel B).

3.3.4 Table 4

In Table 4, RS (2005) seek to rule out the possibility that the positive impact of the newspaper campaign on school total enrolment (which we cannot confirm) is not due to sorting. Indeed, it could be that RS (2005)'s results in column 3 of Panels A and B of Table 3 are driven by the fact that students sort into schools with more resources with in fact no impact of more funding on enrolment at the aggregate level. To rule out this possibility RS, (2005) rely on external data from the work of Björkman (2004) (now Björkman 2007). In this paper, Björkman estimates the impact of the newspaper campaign on enrolment of P7 students at the district level. While it is a possibility that students move to another school in their current school's neighborhood if this alternative school is endowed with more resources, this sorting process is less likely if the unit of observation is not the school but the district. Contrary to RS (2005), Björkman does not use a school's distance to a newspaper outlet in 1997 to determine the probability of being treated. Rather, she uses the number of newspapers per school in a given district, dividing the number of newspapers by the number of schools in a given district. Hence, schools in a given district are assigned the same probability of being treated. Moreover, Björkman does not rely on RS (2005)'s IV two-stage approach but on a reduced form equation which boils down to the following difference-in-difference analysis:

$$y_{jt} = \psi_0 + \psi_1 year_{1997} + \psi_2 newsperschool_j + \psi_3 (newsperschool_j \times year_{1997}) + w_{5jt},$$

for $t = \{1995, 2001\}$. The dependent variable y_{jt} denotes the number of students enrolled in grade P7 in district j at year t. Variable $newsperschool_j$ represents the number of newspaper per school in district j in 1997, when the newspaper campaign started. Variable $year_{1997}$ is defined as in Equation (1). Coefficient ψ_3 is meant to capture the impact of the newspaper campaign on enrolment of P7 students at the district level.

[12] Nor do the results change substantively when relying separately on start-of-year and end-of-year enrolment figures.

Table 4: Impact of the newspaper campaign on enrolment of P7 students: difference-in-differences analysis at the district level

	Dep. var.		
	enroll. of P7 students 1995	enroll. of P7 students 2001	enroll. of P7 students 1995 & 2001
	(1)	(2)	(3)
	Panel A: Original		
Newspapers per school	14.46	34.00*	14.05
	(11.51)	(18.29)	(12.02)
Newspapers per school x 1997			20.16***
			(3.18)
Number of schools	53	53	106
	Panel B: Pure replication		
Newspapers per school	14.46	34.00*	14.05
	(11.51)	(18.29)	(12.02)
Newspapers per school x 1997			20.16***
			(3.18)
Number of schools	53	53	106
	Panel C: Modified replication		
Newspapers per school	14.46	34.00*	
	(11.51)	(18.29)	
Newspapers per school x 1997			21.54***
			(1.74)
District fixed effects	No	No	Yes
Number of schools	53	53	106

Note: Consumption at the district level is included as a control. OLS standard errors are in parentheses, and bootstrapped standard errors are in brackets. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

Panels A and B of Table 4 reports the results from the original paper and its pure replication. In columns 1 and 2, Björkman regresses y_{jt} on $newsperschool_j$ in 1995 and in 2001. If the number of newspapers per school in district j in 1997 is indeed a proxy for the degree of exposure to the newspaper campaign which started in 1997, then the coefficient of this variable should be positive and statistically significant only in column 2 (which concerns the post-treatment period), not in column 1 (which concerns the pre-treatment period). This premise is confirmed by the data. Moreover, column 3 reports the OLS estimates of Equation (9) and reveals a positive and statistically significant coefficient ψ_3. This finding suggests that RS (2005)'s results in column 3 of Panels A and B of Table 3 are not driven by sorting.

In Panel C, we replicate columns 1 and 2 and find the same point estimates. However, we are uncomfortable with the specification of Equation (9) since it does not include district-specific fixed effects. Yet, estimating Equation (9) while holding constant all time invariant characteristics at the district level is a critical first step towards mitigating the risk of an omitted variables bias. Instead of estimating Equation (9) in column 3 of Panel B of Table 4, we therefore estimate Equation (10) by running an OLS district fixed effects estimation:

$$y_{jt} = \psi_{0j} + \psi_1 year_{1997} + \psi_2 (newsperschool_j \times year_{1997}) + w_{6jt},$$

where ψ_{0j} is a district-specific fixed effect which notably absorbs the effect of $newsperschool_j$.

Our estimates show that Björkman (2007)'s results in column 3 of Panel A are robust to controlling for district fixed effects. Not surprisingly, a first-difference OLS estimation provides results that are similar to the OLS district fixed effect estimation.[13] (Results are available upon request.)

Thus, a more demanding approach than the one implemented by Björkman confirms her findings. It would, however, have been more straightforward to test the robustness of RS (2005)'s results to sorting by estimating Equations (3) and (4) at the district level. This we do in the additional analysis.

4. Additional analysis

In their abstract, RS (2005) assert that '[a] newspaper campaign in Uganda aimed at reducing capture of public funds by providing schools (parents) with information to monitor local officials' handling of a large education grant programme. The campaign was highly successful and the reduction in capture had a positive effect on enrolment and student learning.' The pure replication section of this report supports RS (2005)'s claim that the newspaper campaign increases the share of capitation grants reaching schools through improvement of the head teacher's knowledge (see column 1 of Panels B and C of Table 2 and column 2 of Panels B and C of Table 3). However, the pure replication does not confirm that 'the reduction in capture [induced by the newspaper campaign] had a positive effect on enrolment' (see the estimates of Equation (4) in column 3 of Panel C of Table 3), knowing that this result is already weakly statistically significant in RS (2005). This difference stems from the fact that, contrary to RS (2005), we do not discard the 10 schools that experienced a decrease in enrolment. Moreover, RS (2005) do not provide empirical support for their claim that the reduction in capture also has a positive impact on student learning (they simply reference it). We also investigate this outcome, which receives weak support in RS (2011).

Yet, the absence of a robust statistically significant effect of reduced capture on enrolment is not surprising. Since enrolment is provided by the school's head teacher, it can be prone to misreporting. Indeed, head teachers should have more incentive to overestimate enrolment in 2001 (in order to induce a higher capitation grant in the future) in schools which experienced a modest reduction in capture between 1995 is 2001, than in schools where reduction in capture was larger. If so, this misreporting bias runs against finding a positive and statistically significant relationship between an increase in the share of capitation grants reaching a given school and an increase in this school's total enrolment between 1995 and 2001. To avoid this bias, RS (2011) rely on actual (not self-reported) enrolment figures when estimating the impact of a decrease in capture due to the newspaper campaign on enrolment outcomes. From now on, we therefore rely on enrolment of P7 students rather

[13] This approach consists in differencing Equation (10) in order to eliminate district-specific fixed effects. A first-difference OLS estimation therefore relies on the following Equation (11): $\Delta y_j = y_{j,2001} - y_{j,1995} = \chi_0 + \chi_1 newsperschool_j + \Delta w_{6j}$.

than on total enrolment. These data are provided by the Primary Leaving Exam (PLE) records from the Uganda National Examination Bureau. In this context, enrolment of P7 students is simply equal to the number of P7 students actually taking the PLE.

The purpose of our additional analysis is first to test the robustness of the impact of the newspaper campaign on capitation grant capture. We then test the robustness of the newspaper campaign on enrolment and learning outcomes. Finally, we investigate the relationship between distance to a newspaper outlet and change in PTA charges between 1995 and 2001, change in teacher salaries and change in the number of teachers. Should any of these relationships be shown to be statistically significant, then controlling for them in the original estimations is critical. Thus, as the change in the number of teachers proves to be important in our later analysis, we include it as an additional control throughout the following sections.

4.1 Impact of the newspaper campaign on capture

Column 2 of Table 3 showed a negative and statistically significant correlation between 'Distance to a newspaper outlet' and the difference in the share of capitation grants reaching the school. In this section, we aim to ensure that this correlation indeed measures the impact of the newspaper campaign on capture by showing (i) that the parallel trend assumption appears to be satisfied and (ii) that the correlation between distance to a newspaper outlet and change in the share of capitation grants reaching the school remains statistically significant when one controls for correlates of distance to a newspaper outlet whose impact on capture was likely exacerbated after UPE in 1997.

4.1.1 Testing the parallel trend assumption

For $\gamma_1$in Equation (3) to measure the impact of the newspaper campaign on the capture of capitation grants, one must show that the impact of a change in time-varying characteristics is similar *prior to the newspaper campaign* in schools with a high probability of being exposed to the newspaper campaign (due to their proximity to a newspaper outlet) and in schools with a low probability of being exposed to this campaign (due to their distance from a newspaper outlet). In other words, a school's distance to a newspaper outlet should not be correlated with the change in the share of capitation grants reaching the school before 1997.

Columns 1 and 2 of Table 5 report the correlation between distance to a newspaper outlet and the change in the share of capitation grants reaching the school over the 1991−1995 and 1995−2001 periods, respectively. The results confirm that this correlation is negative and statistically significant only after the launch of the newspaper campaign (this correlation is positive and not statistically significant during the pre-campaign period). The data are therefore supportive of the parallel trend assumption.

It is important to stress that RS (2011) also provide support for the parallel trend assumption in column 1 of their Table 4 which focuses on the 1991−1995 period. However, information on some of the schools in their sample is available for only one of the two years in this period (either in 1991 or in 1995). This explains why RS (2011) rely on a fixed effect estimation for the 1991−1995 period (while they rely on a first-difference estimation for the 1995−2001 period to neutralise school fixed effects). However, there is no guarantee that their results from a fixed effect estimation with unbalanced panel data would coincide with those derived from a fixed effect estimation with balanced panel data (or, equivalently, a

first-difference estimation). This is due to the fact that, while both schools surveyed twice and schools surveyed only once enter their estimation, fixed effects for the latter type of schools are not identified. Hence, this feature may compromise the reliability of the RS (2011)'s test of the parallel trend assumption.

Table 5: Distance to a newspaper outlet and change in the share of capitation grants

	Dep. var.	
	Δs	Δs
	91-95	95-01
	(1)	(2)
Distance to a newspaper outlet	0.81	-6.77**
	(2.28)	(2.62)
Number of schools	147	199

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.1.2 Controlling for competing channels

To increase confidence that γ_1 in Equation (3) measures the impact of the newspaper campaign on the capture of capitation grants, testing the parallel trend assumption is not sufficient. One must also ensure that γ_1 remains statistically significant when one controls for competing channels, chief of which being the implementation of UPE. As already stressed, the UPE reform occurred at nearly the same time as the newspaper campaign (the UPE reform preceded the newspaper campaign by roughly 10 months). The UPE reform led to the abolition of tuition fees (paid by parents) and parent-teacher association (PTA) charges for primary education. As such, $\gamma_1$may simply capture the impact of the UPE reform on capture. Why? The abolition of primary school fees should push all local communities to better secure the transfer of capitation grants disbursed by the central government since, starting from 1997, they constitute the major source of school funding (in addition to the school facilities grants also disbursed by the central government). However, the ability and incentives (beyond the necessity to compensate for the abolition of school fees following the UPE reform) of local communities to improve their share of funding is likely to be negatively correlated with distance to a newspaper outlet. The risk for γ_1 in Equation (3) to lose statistical significance when one controls for local communities' ability and incentive to mobilise could serve as an additional test.

To illustrate this claim, we first focus on the local community's ability to secure the transfer of capitation grants. RS (2004) study the share of capitation grants reaching the school during the pre-campaign period from 1991 to 1995. They notably show that better-off communities manage to claim a higher share of their entitlements. This finding suggests that ‘rather than being passive recipients of flows from the government, schools use their bargaining power to secure greater shares of funding (p. 679),’ a behaviour likely to increase after the UPE reform. Yet, as shown by Table 6, the correlation between distance to a newspaper outlet and consumption at the district level in 1995 (a proxy for the local community's wealth prior to 1997) is negative and statistically significant. This suggests that γ_1 suffers from a downwards bias which requires that we control in Equation (3) (as well as in Equation [4]) for

consumption in 1995. Table 6 also shows that the ability to communicate with local officials is similarly negatively correlated to distance to a newspaper outlet. This ability is captured by three proxies: (i) the school's proximity to an urban centre; (ii) the presence of a local official or of a representative of the District Education Office (DEO) in the School Management Committee (SMC); and (iii) whether the school has received discretionary financial support.[14] More precisely, the latter variable captures whether the school has received financial support from sources other than government or Parents and Teachers Association, hence from local officials. Controlling in Equation (3) for these additional variables is therefore critical.

Table 6: Correlation between distance to a newspaper outlet and local community's ability and incentives to exert pressure on local officials

	Distance to a newspaper outlet
Ability to secure grants	
Wealth in 1995	-0.42***
Proximity to an urban centre	-0.58***
Local official or representative of the DEO in the SMC	-0.09*
Discretionary financial support	-0.18***
Incentive to secure grants	
Capture of capitation grants in 1995	-0.13***
Enrolment of P7 students in 1995	-0.31***
PLE scores in 1995	-0.31***

Note: [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

We now focus on the local community's incentives (beyond the mere existence of the UPE reform) to secure the transfer of capitation grants. One can think of three reasons: first, the greater the capture of funding reaching the school in 1995, the greater the incentive to mobilise. Second, schools where enrolment figures (we focus on enrolment in grade 7) are higher prior to the newspaper campaign should also be more prone to secure the transfer of capitation grants since they were more in need of these grants after the abolition of the tuition fees that followed the UPE reform. Indeed, controlling for consumption at the district level (done in all of the original and replicated estimations), it is likely that the ability of local communities to go on providing full financial support to schools after this reform decreases with the student body's size. Similar reasoning may apply to schools showing higher PLE scores in 1995: the higher the initial quality of those schools, the greater the incentive of the local community to secure capitation grants after the UPE reform in order to maintain the school's quality.

[14] The school's proximity to an urban centre derives from question Q30 of the 2002 PETS which asks the head teacher to report the 'distance to the nearest bank branch (in km).' We use the natural logarithm of one plus this distance. The presence of a local official or of a representative of the DEO in the SMC derives from questions Q52A and Q52B of the 2002 PETS which asks the head teacher to specify whether the 'local council (LC)' (Q52A) and whether the 'DEO's office' (Q52B) 'is represented in the SMC.' Finally, whether the school has received discretionary financial support derives from question Q84 of the 2002 PETS, which asks the head teacher to specify the amount he/she received in 2001 from various sources (government, PTA and other sources). The underlying assumption is that these variables, which are measured for year 2001, are acceptable proxies for the same characteristics in 1997.

Yet, as revealed by Table 6, all three variables (the degree of capture of capitation grants in 1995, the number of P7 students enrolled in 1995 and their PLE scores in 1995) are negatively correlated with the school's distance to a newspaper outlet. It is therefore critical to ensure that γ_1 in Equation (3) remains statistically significant when one purges γ_1 of its downwards bias by controlling for these variables.

Table 7 displays the OLS coefficients of an estimation which regresses the change in the share of capitation grants reaching the school between 1995 and 2001 on distance to a newspaper outlet in 1997 while holding constant, sequentially, each of the seven proxies for local communities' ability and incentives to mobilise. Our results reveal that γ_1 remains negative and statistically significant after controlling for the UPE reform channel.

Table 7: Distance to a newspaper outlet and change in the share of capitation grants when one controls for the UPE reform channel

	Dep. var.: Δs						
	95-01	95-01	95-01	95-01	95-01	95-01	95-01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Distance to a newspaper outlet	-3.92[a]	-4.90[a]	-6.42**	-6.45**	-2.80*	-6.63**	-6.68**
	(2.78)	(3.03)	(2.66)	(2.64)	(1.66)	(2.89)	(3.06)
Wealth in 1995	0.000***						
	(0.00)						
Distance to an urban centre		-3.67					
		(3.37)					
Local official or representative of the DEO in the SMC			17.31[a]				
			(11.47)				
Discretionary financial support				8.33			
				(6.07)			
Share of capitation grants in 1995					-0.84***		
					(0.05)		
Enrolment of P7 students in 1995						0.16***	
						(0.06)	
PLE scores in 1995							0.98*
							(0.56)
Number of schools	199	199	199	199	199	176	176

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

One might argue that an additional proxy for local communities' incentive to mobilise would be their awareness of the existence of the UPE reform. Yet, this awareness may be negatively correlated with distance to a newspaper outlet. We unfortunately do not have a proxy for local communities' knowledge of the UPE reform. But we doubt that this knowledge depends negatively on distance to a newspaper outlet. First, this information is far too critical to be learned by head teachers through newspapers. And indeed, relying on question Q57 of

the 2002 PETS[15], all of the 388 head teachers surveyed in 2002 were aware that his/her school was entitled to a UPE capitation grant. Second, the introduction of UPE was not only broadcast by newspaper but also by other media[16] such as the radio, whose coverage in Uganda in the late 1990s was already broad (Deininger 2003). This context limits the likelihood that there is a negative correlation between distance to a newspaper outlet and awareness of the introduction of the UPE reform. We provide additional empirical support for this claim in section 4.4.

It is important to stress that RS (2011) only control in columns 1 to 3 of their Table 9 for three of the seven proxies mentioned above. These three proxies only concern the local communities' ability to mobilise: (i) the distance to an urban centre; (ii) whether a local official is present in the SMC (however, contrary to ours, this dummy does not reflect the presence of a representative of the DEO in addition to the presence of the local officer); (iii) whether the school has received discretionary financial support.

Overall, our approach for controlling for the UPE reform channel, though straightforward, is more exhaustive than the one followed by RS (2011) or their 2006 working paper. Yet, it does not affect the negative and statistically significant coefficient of $distance_i$ in Equation (3). We therefore believe that our additional analysis helps to increase confidence that this coefficient does measure, as RS (2005 and 2011) claim, the impact of the newspaper campaign on capture of capitation grants.

4.2 Impact of the newspaper campaign on enrolment of P7 students

In this section we first present the estimates of Equations (3) and (4) when Δy_i in Equation (4) represents the change in enrolment of P7 students between 1995 and 2001. We then test the robustness of these results for sorting and the support of the exclusion restriction.

4.2.1 Estimating Equations (3) and (4)

Table 8 reports the IV estimates of Equations (3) and (4) when Δy_i in Equation (4) represents the change in enrolment of P7 students from 1995 to 2001. Results from the first-stage (column 1) confirm the negative and strongly significant relationship between 'Distance to a newspaper outlet' and Δs. Moreover, the second-stage (column 2) reveals that an increase in the share of capitation grants reaching the school due to the newspaper campaign has a positive and statistically significant impact on the change in enrolment of P7 students. In the absence of misreporting bias, empirical evidence is therefore supportive of the fact that reduced capture of capitation grants and increased school resources due to the newspaper campaign improves enrolment outcomes.[17] Columns 3 through 8 of Table 8 show that these results are robust to controlling for the change in PLE scores of P7 students and the change in the number of teachers, which could generated omitted variable bias if excluded. (We elaborate on the impact of the newspaper campaign on the number of teachers in Section 4.4.)

[15] Question Q57 asks the head teacher whether his/her 'school [is] entitled to UPE capitation grant.'
[16] In contrast, only two national newspapers published information on the amount and timing of capitation grants: The Monitor and The New Vision.
[17] Results reported in columns 1 and 2 to Table 8 are similar to those reported by RS (2011) in columns 1 and 4 of their Table 7. The difference is due to the fact that RS (2011) rely on the *hadimvo* command on Stata in order to identify outliers. This leads them to drop more observations than we do (they estimate Equations [3] and [4] on 166 observations while we rely on all 171 observations to perform this analysis).

Yet, results from the second-stage, computed at the school level, could be due to sorting. As emphasised in section 3.3.4, students may sort into schools with more resources with in fact no impact of more funding on enrolment at the aggregate level. Moreover, results from the second-stage may be due to a violation of the exclusion restriction: distance of school i to a newspaper outlet in 1997 may not impact the change in enrolment (only) through the newspaper campaign. We address these two issues in sections 4.2.2 and 4.2.3.

Table 8: Impact of the newspaper campaign on enrolment of P7 students

	Dep. var.							
	Δs	Δ P7 enroll.	Δs	Δ P7 enroll.	Δs	Δ P7 enroll.	Δs	Δ P7 enroll.
	95-01	95-01	95-01	95-01	95-01	95-01	95-01	95-01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to a newspaper outlet	-8.43***		-8.45***		-8.36***		-8.39***	
	(3.03)		(3.03)		(3.06)		(3.05)	
$\widehat{\Delta s}$		0.68**		0.68**		1.09**		0.85**
		(0.33)		(0.34)		(0.50)		(0.34)
Δ PLE scores			-0.36	-0.80			-0.32	-1.00[a]
			(0.69)	(0.63)			(0.69)	(0.70)
Δ nb teach.					-0.21	1.63[a]	-0.18	1.92*
					(0.52)	(1.12)	(0.52)	(1.10)
Number of schools	171	171	171	171	170	170	170	170

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.2.2 Testing for sorting

Table 9 is similar to columns 7 and 8 of Table 8 with the exception that Equations (3) and (4) are estimated at the district level using the mean values of the dependent and explanatory variables. Results from Table 8 are fully confirmed, although the point estimate for the coefficient of $\widehat{\Delta s}$ is lower when the analysis is conducted at the district level. This finding suggests that sorting may be at stake. However, the fact that the coefficient of $\widehat{\Delta s}$ remains positive and statistically significant in Table 9 reveals that sorting does not explain the entire effect of the results estimated in Table 8. Although not a perfect test, this is an important finding that RS (2005) did not fully provide. As stressed in Section 3.3.4, RS (2005) test for sorting based on Björkman (2004)'s approach, which differs from their estimation strategy and do not thoroughly address sorting in their 2011 paper. This is because, as they explain on page 965, their 'sample consists almost exclusively of rural schools and the pool of potential students served by these schools typically does not have much choice with respect to the primary school to attend.' However, RS (2011) do ensure that their results (those similar to the ones provided in columns 1 and 2 of our Table 8) hold when they control for the distance to the closest school the students could attend. This strategy mitigates the risk that schools which received a higher share of their capitation grants due to their proximity to a newspaper outlet are also the ones in which it was easiest for students to sort (i.e.

distance to a newspaper outlet and distance to the closest school the students could attend are correlated). But sorting can still occur even when it requires more effort to change schools. We therefore believe that estimating Equations (3) and (4) at the district level addresses this issue more thoroughly.

Table 9: Impact of the newspaper campaign on enrolment of P7 students: analysis at the district level

	Dep. var.	
	Δs	Δ P7 enroll.
	95-01	95-01
	(1)	(2)
Distance to a newspaper outlet	-22.47**	
	(9.02)	
$\widehat{\Delta s}$		0.74**
		(0.37)
Δ PLE scores	-1.41	-1.38**
	(1.20)	(0.67)
Δ nb teach.	-1.68	2.51
	(3.58)	(3.24)
Number of districts	21	21

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.2.3 Testing support for the exclusion restriction

Providing support for the exclusion restriction requires showing that distance to a newspaper outlet has a statistically significant impact on enrolment of P7 students only through the newspaper campaign. In other words, the coefficient of distance to a newspaper outlet should be statistically significant only over the 1995−2001 period which is partly post-campaign. We investigate the exclusion restriction by comparing the relationship between distance to a newspaper outlet and change in enrolment of P7 students over the 1991−1995 period (column 1 of Table 10) and the 1995−2001 period (column 2 of Table 10). We add controls step-wise in the remaining columns three through eight.

Table 10 reveals a negative and statistically significant impact of distance to a newspaper outlet on the change in enrolment in both columns one and two. Put differently, the exclusion restriction is violated. Working on balanced panel data does not allow us to confirm the results reported in column two of RS (2011)'s Table 4 which show an insignificant relationship between distance to a newspaper outlet and change in enrolment during the pre-campaign period. However, as soon as we control for the change in the number of teachers (column 5), the coefficient of distance to a newspaper outlet loses statistical significance for the 1991−1995 period while it remains statistically significant for the 1995−2001 period. The inclusion of this control treats omitted variables bias since, as emphasised and shown in section 4.4, the change in the number of teachers is correlated to both distance to a newspaper outlet and the change in the number of P7 students. Treating this omitted variables bias is key to providing support for the exclusion restriction, as shown in columns five to eight of Table 10.

Table 10: Distance to a newspaper outlet and change in enrolment of P7 students

	Dep. var.: Δ P7 enroll.							
	91-95	95-01	91-95	95-01	91-95	95-01	91-95	95-01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Distance to a newspaper outlet	-2.52*	-3.85**	-2.62*	-3.96**	-2.33	-5.60***	-2.33	-5.88***
	(1.51)	(1.84)	(1.51)	(1.86)	(1.94)	(1.99)	(1.95)	(1.95)
Δ PLE scores			-0.18	-0.34			-0.13	-1.40***
			(0.22)	(0.32)			(0.29)	(0.47)
Δ nb teach.					2.09***	1.54*	2.11***	1.66*
					(0.62)	(0.84)	(0.62)	(0.85)
Number of schools	233	281	233	281	117	181	117	181

Note: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.3 Impact of the newspaper campaign on PLE scores of P7 students

In this section we first present the IV estimates of Equations (3) and (4) when Δy_i in Equation (4) represents the change in PLE scores of P7 students between 1995 and 2001. We then test the robustness of these results for sorting and the validity of the exclusion restriction.

4.3.1 Estimating Equations (3) and (4)

Column 1 of Table 11 reports the IV estimates of Equation (4) when Δy represents the change in PLE scores of P7 students from 1995 to 2001. Results from the second-stage reveal no statistically significant impact of the change in the share of capitation grants reaching the school on the PLE scores of P7 students (the coefficient of $\widehat{\Delta s}$ is far from statistical significance with a p-value equal to 0.885). As such, our results are consistent with those reported by RS (2011) in column 1 of their Table 8. As already mentioned, RS (2011) only show with their reduced form approach that there is an effect. More precisely, this reduced form approach consists in a fixed-effect estimation with unbalanced panel data covering years 1995, 1997, 2001 and 2002 in which PLE score of P7 students is regressed on distance to a newspaper outlet. The coefficient of distance to a newspaper outlet is negative and statistically significant (at the 90 per cent confidence level).[18] However, this reduced form approach offers no guarantee that the channel driving these results is an increase in capitation grants due to the newspaper campaign.

There are three variables which, if controlled for in Equations (3) and (4), could improve the statistical significance of the coefficient of $\widehat{\Delta s}$ in Equation (4). First, not controlling for the change in enrolment of P7 students may bias this coefficient downwards. Indeed, the change in enrolment is positively correlated with $\widehat{\Delta s}$ but possibly negatively correlated with the change in PLE scores. An increase in the number of students might indeed translate into more strain on resources and therefore lower academic achievement. Moreover, it may

[18] Note that, for this statistically significant result to emerge, RS (2011) need to control besides for region fixed-effects, an approach which is not the default in the rest of their paper.

coincide with an influx of lower-achieving students. Second, omitting the change in the number of teachers in Equation (4) may also lead to underestimating the positive impact of $\widehat{\Delta s}$ on the change in PLE scores (through its positive correlation with distance to a newspaper outlet that we document in section 4.4). Indeed, the change in the number of teachers is negatively correlated with $\widehat{\Delta s}$ but presumably positively correlated with the change in PLE scores. Finally, it seems critical to control in Equations (3) and (4) for the PLE scores of P7 students in 1995. Indeed, contrary to the other dependent variables studied in this replication report – as well as in RS (2005 and 2011) – PLE scores are bounded from above; they cannot exceed 36. Put differently, the higher the PLE score in 1995, the lower the increase in PLE scores between 1995 and 2001. The data confirm that this negative correlation is equal to 59 per cent and statistically significant at the 99 per cent confidence level. Yet, PLE scores in 1995 are positively correlated with $\widehat{\Delta s}$. Consequently, this pattern is again likely to bias the coefficient of this variable downwards and may mistakenly lead us to fail to reject the null.

Columns 2 through 5 of Table 11 therefore report the second-stage OLS estimates when we control, sequentially, for the change in enrolment of P7 students (column 2), for the change in the number of teachers (column 3) and for the PLE score in 1995 (column 4). Column 5 controls for all these variables together. Our results reveal that the coefficient of $\widehat{\Delta s}$ becomes statistically significant only when we control for PLE scores in 1995 (see column 4 of Table 11). This result is robust to controlling for the change in enrolment of P7 students and their PLE scores, as shown in column 5 of Table 11. The p-value of the coefficient of $\widehat{\Delta s}$ is indeed equal to 0.100 in this case. Note that all of the first-stage estimates confirm the statistically significant negative relationship between Δs and distance to a newspaper outlet.

Table 11: Impact of the newspaper campaign on PLE scores of P7 students

	Dep. var.: PLE scores				
	95−01	95−01	95−01	95−01	95−01
	(1)	(2)	(3)	(4)	(5)
$\widehat{\Delta s}$	0.01	0.03	0.01	0.12*	0.16[a]
	(0.04)	(0.05)	(0.04)	(0.07)	(0.10)
Δ P7 enroll.		-0.03[a]			-0.05*
		(0.02)			(0.03)
Δ nb teach.			0.09*		0.08
			(0.05)		(0.12)
PLE score in 1995				-0.69***	-0.72***
				(0.13)	(0.16)
Number of schools	171	171	170	171	170

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

Yet, results from column 5 of Table 11 which are computed at the school level could be due to sorting if higher-achieving students may sort into schools with more resources. Moreover, they may be due to a violation of the exclusion restriction. We address these two issues in sections 4.3.2 and 4.3.3.

4.3.2 Testing for sorting

Table 12 reports the first- and second-stage estimates displayed in column 5 of Table 11 when these estimates are computed at the district level. The coefficient of $\widehat{\Delta s}$ gains statistical significance and magnitude compared to column 5 of Table 11. Results reported in Table 11 are therefore not driven by higher-achieving students joining schools receiving a higher share of their capitation grants.

Table 12: Impact of the newspaper campaign on PLE scores of P7 students: analysis at the district level

	Dep. var.	
	Δs	Δ PLE scores
	95-01	95-01
	(1)	(2)
Distance to a newspaper outlet	-15.30**	
	(5.56)	
$\widehat{\Delta s}$		0.27**
		(0.11)
Δ PLE enroll.	0.26	-0.18*
	(0.32)	(0.09)
Δ nb teach.	-2.18	1.00
	(3.89)	(1.06)
PLE score in 1995	0.65	-0.61[a]
	(1.78)	(0.41)
Number of districts	21	21

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.3.3 Testing support for the exclusion restriction

Providing support for the exclusion restriction requires showing that distance to a newspaper outlet has a statistically significant impact on the PLE scores of P7 students only through the newspaper campaign. In other words, the coefficient of distance to a newspaper outlet should be statistically significant only over the 1995−2001 period which is partly post-campaign. We investigate the exclusion restriction by comparing the relationship between distance to a newspaper outlet and change in PLE scores over the 1991−1995 period (column 1 of Table 13) and over the 1995−2001 period (column 2 of Table 13).

Table 13: Distance to a newspaper outlet and change in PLE scores of P7 students

	Dep. var.: Δ PLE scores									
	91-95	95-01	91-95	95-01	91-95	95-01	91-95	95-01	91-95	95-01
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Distance to a newspaper outlet	-0.60*	-0.34	-0.63*	-0.39	-0.04	-0.20	-1.12***	-1.23***	-0.73*	-1.02***
	(0.34)	(0.30)	(0.35)	(0.30)	(0.49)	(0.36)	(0.29)	(0.25)	(0.41)	(0.29)
Δ PLE enroll.			-0.01	-0.01					0.03[a]	-0.03***
			(0.02)	(0.01)					(0.02)	(0.01)
Δ nb teach.					0.13	0.09**			-0.03	0.03
					(0.11)	(0.04)			(0.11)	(0.05)
Initial PLE scores							-0.62***	-0.67***	-0.61***	-0.56***
							(0.06)	(0.05)	(0.10)	(0.07)
Number of schools	233	281	233	281	117	181	233	281	117	181

Note: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

Table 13 reveals a negative and statistically significant impact of distance to a newspaper outlet on the change in PLE scores in column 1. Again, working on balanced panel data does not allow us to confirm the results reported by RS (2011) in column 3 of their Table 4 which shows an insignificant relationship between distance to a newspaper outlet and change in PLE scores during the pre-campaign period. The exclusion restriction remains violated even when we control simultaneously for the change in enrolment of P7 students, for the change in the number of teachers and for PLE scores in 1991 (see column 9 of Table 13).[19] Put differently, it is likely that the result reported in column 5 of Table 11 which reveals a positive and statistically significant impact of $\widehat{\Delta s}$ on the change in PLE scores between 1995 and 2001 is driven by omitted variables correlated to distance to a newspaper outlet. Obvious candidates are those factors which capture the level of development and/or the remoteness of the neighborhood in which the school is located, such as the district's consumption level in 1995 and the school's distance to an urban centre.

In Table 14 we therefore test whether the results reported in column 5 of Table 11 are robust to controlling, sequentially, for these two variables. They are not. The coefficient of $\widehat{\Delta s}$ is no longer statistically significant. Consequently, we cannot confirm RS (2005 and 2011)'s claim that an increase in the share of capitation grants reaching the school due to the newspaper campaign improves learning outcomes.

[19] It is also violated when we control for the change in enrolment of P7 students only (column 3 of Table 13) or for initial PLE scores only (column 7 of Table 13).

Table 14: Impact of the newspaper campaign on PLE scores of P7 students: Controlling for competing channels

	Dep. var.: Δ P7 scores	
	(1)	(2)
$\widehat{\Delta s}$	0.21	0.11
	(0.19)	(0.11)
Δ P7 enroll.	-0.06[a]	-0.05*
	(0.04)	(0.03)
Δ nb teach.	0.05	0.07
	(0.12)	(0.10)
PLE score in 1995	-0.72***	-0.71***
	(0.20)	(0.13)
Wealth in 1995	-0.00	
	(0.00)	
Distance to an urban centre		-0.69
		(0.77)
Number of schools	170	170

Note: Δs represents the change in the share of entitled capitation grants received by a school between 1995 and 2001. Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

4.4 Impact of distance to a newspaper outlet on complementary school characteristics

This section investigates if there are other educational characteristics that may be affected by proximity to a newspaper outlet (through the newspaper campaign). In our replication plan, we proposed to investigate additional educational characteristics that might have relevance to the robustness and interpretation of the results. Though we had hoped to test a more exhaustive set of factors, which was not feasible due to data constraints, we instead use only the variables available in the data across both PETS: PTA contributions, teacher salaries and the number of teachers.

4.4.1 Speculating on the impact

The introduction of UPE led to the abolition of tuition fees (paid by parents) and of PTA charges for primary education.[20] Could the overall decrease in PTA fees between 1995 and 2001 be affected by the distance to a newspaper outlet? This might occur if parents in schools closer to a newspaper outlet are more likely to be informed of the UPE reform and aware of the fact that they are no longer expected to contribute to the PTA. However, as we already mentioned, knowledge of the UPE reform was disseminated through a variety of media sources, not only newspapers. We therefore suspect that parents' knowledge about

[20] Historically, these PTA charges were introduced during the 1970s to complement the low salaries of teachers. As stressed in ODI (2006), 'collections from PTA charges were used as an incentive for teachers and also for the general running of a school. Parents and teachers of respective schools would agree on the amount, which varied from school to school.' To compensate schools for the loss in tuition fees, capitation grants were doubled, as discussed in the 'Pure replication' section of this report, and teacher salaries were increased.

the UPE reform is orthogonal to their distance to a newspaper outlet and that the decrease in PTA contributions between 1995 and 2001 is no different in schools closer to a newspaper outlet.[21]

Turning to the change in teacher salaries between 1995 and 2001, could it be affected by distance to a newspaper outlet? On one hand, the newspaper campaign might have negatively affected salaries. This campaign aimed to create greater transparency with respect to the transfer of capitation grants but not with respect to other types of funding such as teacher salaries. The lower pressure for transparency regarding other types of transfers from the government is well reflected by the 2002 PETS. Over the 388 schools, 84 per cent publicly display UPE capitation grant received from the district.[22] By contrast, only a minority (30 per cent) proceed in a similar way regarding total teachers' salaries received from the district (school facilities grants are not covered by the 2002 PETS).[23] It could therefore be that local officials decided to compensate themselves for not being able to capture capitation grants for schools located closer to a newspaper outlet, hence by capturing teachers' salaries. If, however, the newspaper campaign induced treated schools to mobilise in order to receive *all* of the funds to which they are entitled (teacher salaries included), this would suggest a negative relationship between the change in teacher salaries between 1995 and 2001 and distance to a newspaper outlet. In other words, the newspaper campaign might have positively affected salaries. But it is also likely that teacher salaries were not among the public funds monitored by local officials. Indeed, as RS (2004) describe, the central government held responsibility for the teacher payroll which implied a higher level of oversight. Moreover, teachers were assuredly better informed about the value of their own salaries than were head teachers about the value of their capitation grants before the newspaper campaign. This leaves less room for capture of teacher salaries by local officials. If so, we should expect orthogonality between the change in teacher salaries from 1995 to 2001 and distance to a newspaper outlet.[24]

The district government held responsibility for hiring and placement decisions once a teacher was recruited by the central government. In other words, a teacher theoretically had no choice on their allocation to a school. Evidence shows that, prior to the campaign, the number of teachers in a given school was negatively and significantly correlated with distance to a newspaper outlet, even when one accounts for the number of students enrolled. (Results are available upon request.) This suggests that schools closer to a newspaper outlet had more than their fair share of teachers. Yet, those schools are not among the poorest (since we show in Table 6 that wealth in 1995 is negatively and significantly correlated with distance to a newspaper outlet). This implies that the unequal allocation was not followed with the goal of helping disadvantaged schools.

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[21] Obviously, the change in PTA fees may be indirectly affected by distance to a newspaper outlet through this distance's impact on the change in the number of teachers (for instance, a higher number of teachers may be more able to exert pressure on parents so that they maintain their PTA contributions). In section 4.4.2 we show that our results on the change in PTA fees are robust when we control for the change in the number of teachers.

[22] This information is based on question Q76c of the 2002 PETS: 'Do you display publicly UPE capitation grant received?'

[23] This information is based on question Q76b of the 2002 PETS: 'Do you display publicly total teachers' salaries received from the district?'

[24] Again, we ensure in section 4.4.2 that our results on the change in teacher salaries between 1995 and 2001 are robust when we control for the change in the number of teachers.

This inequality rather suggests that, prior to the newspaper campaign, district officials in charge of placement may have sorted teachers due to other reasons: pressure of teachers themselves anxious to avoid being posted in remote areas or pressure of schools to get as many teachers as possible, with less remote schools better able to lobby, bribe, etc. How could the newspaper campaign have changed this pattern? We know that the newspaper campaign substantially reduced local officials' ability to capture public funds. This might have induced the most corrupt officials (i.e. those the most prone to cede to bribes/pressures) to change their behaviour or leave. If so, then we should observe that the newspaper campaign allowed for a fairer allocation of teachers across schools. More precisely, the change in the number of teachers between 1995 and 2001 should positively depend on the distance to a newspaper outlet.[25] Section 4.4.2 tests the assumptions on the impact of distance to a newspaper outlet on complementary school characteristics that we have just developed.

4.4.2 Testing the impact

Columns 1 and 2 of Table 15 report the OLS coefficients of an estimation which regresses the change in PTA contributions on distance to a newspaper outlet.[26] Column 1 reports the results when the change in PTA contributions is computed over the 1991−1995 period while column 2 concerns this change over the 1995−2001 period. Column 1 shows that distance to a newspaper outlet is orthogonal to the change in PTA fees prior to the UPE reform. Column 2 of Table 15 reveals that this relationship is unaffected by the UPE reform: the decrease in PTA contributions between 1995 and 2001 is no different in schools closer to a newspaper outlet. This result provides additional evidence that parents' knowledge about the UPE reform is orthogonal to their distance to a newspaper outlet.

[25] Obviously, the change in the number of teachers may be indirectly affected by distance to a newspaper outlet through this distance's impact on the change in (i) PTA fees; (ii) teacher salaries; and (iii) enrolment. In section 4.4.2 we show that our results on the change in the number of teachers are robust when we control for these variables.

[26] The value of PTA charges in 1995 derives from question QB7 of the 1996 PETS ('Total PTA levies collected') while the value of PTA charges in 2001 derives from question Q84d of the 2002 PETS ('Total PTA fees'). We warmly thank Bernard Gauthier for sharing with us the data for question QB7 of the 1996 PETS.}

Table 15: Distance to a newspaper outlet and complementary school characteristics

	Dep. var.					
	Δ PTA charges 91-95	Δ PTA charges 95-01	Δ teach. sal. 91-95	Δ teach. sal. 95-01	Δ nb teach. 91-95	Δ nb teach. 95-01
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to a newspaper outlet	-0.22	-0.43	0.86*	-0.05	-0.22	0.80**
	(0.38)	(0.45)	(0.49)	(0.37)	(0.34)	(0.37)
Robust to controlling for						
Δ nb teach.	yes	yes	yes	yes	no	no
Δ PTA charges	no	no	no	no	yes	yes
Δ teach. sal.	no	no	no	no	yes	yes
Δ P7 enroll.	no	no	no	no	yes	yes
Number of schools	153	212	153	212	152	212

Note: Change in consumption at the district level is included as a control. OLS robust standard errors are in parentheses. [a], *, ** and *** indicate statistical significance at the 85% ($p < 0.15$), 90% ($p < 0.10$), 95% ($p < 0.05$) and 99% ($p < 0.01$) confidence levels, respectively.

In columns 3 and 4 of Table 15, we analyse the impact of distance to a newspaper outlet on the change in teachers' salaries from the central government.[27] Column 3 focuses on the 1991−1995 period while column 4 focuses on the 1995−2001 period. Prior to the newspaper campaign, there is a positive relationship between the change in teacher salaries and distance to a newspaper outlet (column 3). However, this relationship becomes negative (though not statistically significant) after the campaign (column 4 of Table 15). This suggests that, if anything, the newspaper campaign induced treated schools to mobilise in order to receive all of the funds to which they are entitled (teacher salaries included), thereby countering the positive relationship between the change in teacher salaries and distance to a newspaper outlet that is observed prior to the campaign.

In columns 5 and 6, we analyse the impact of distance to a newspaper outlet on the change in the number of teachers over the 1991−1995 and 1995−2001 periods, respectively. Distance to a newspaper outlet is orthogonal to the change in the number of teachers prior to the newspaper campaign (column 5 of Table 15). However, after the campaign, the relationship between these two dimensions becomes statistically significant and positive (column 6). This suggests that the newspaper campaign induced the most corrupt local officials to change their behaviour or leave and generated a fairer allocation of teachers across schools (since, as already emphasised, schools closer to a newspaper outlet had more than their fair share of teachers prior to the campaign).

[27] The value of teachers' salaries from the central government in 1995 derives from question QC1 of the 1996 PETS ('Total teachers' salary and allowances from the government') while the value of teachers' salaries from the central government in 2001 derives from question Q84b of the 2002 PETS (Total teachers' salaries (incl. HT (head teacher) from the government). We warmly thank Bernard Gauthier for sharing with us the labelled data and questionnaires for question QC1 of the 1996 PETS.

5. Discussion

This replication report confirms the key finding of RS (2005): that the newspaper campaign launched in Uganda in 1997 reduced the capture of government capitations grants transferred to schools by increasing information and awareness among head teachers and parents. We find that a one standard deviation (2.16 km) decrease in distance to a newspaper outlet increases the share of funding that reaches a school by 0.24 standard deviations or roughly 10 percentage points (Table 8, column 7). In their column 1 of Table 7, RS (2011) document an increase in funding of 0.23 standard deviations or roughly 9 percentage points. This result is, arguably, the most policy-relevant of the original paper because it shows that citizen-focused campaigns can make a difference in reducing corruption in public service delivery.

The Ugandan case can be compared to similar changes in educational financing in Madagascar: in 2002, the government of Madagascar overhauled education financing by covering the tuition fees of all students in public primary schools. A number of monitoring devices accompanied this policy change. The government (i) required schools to post the amount of money received (though only 29 per cent of schools complied) and submit budget plans, (ii) imposed district-level audits (though anecdotal evidence suggests those schools affected were closest to the capital), and (iii) promoted the campaign via mass media (newspapers, radio and television). Francken *et al.* (2009) show that remoteness (and therefore less top-down monitoring) increased the amount of capture while access to information for bottom-up monitoring was important for reducing capture. Though the authors do not study the response of educational outcomes to changes in capture, these results are in line with the conclusions of RS (2005 and 2011).

Whether additional funding improves educational outcomes is a more complicated issue. We provide support for RS (2005 and 2011)'s conclusion that increasing the share of funding allocated to school resources improves enrolment. RS (2011) find that a one standard deviation increase in the share of funding reaching the school increases enrolment of P7 students by 1.2 standard deviations or 29 students (their column 4 of Table 7). Our results are extremely similar: we find an increase in enrolment of 1.1 standard deviations or 24 students (Table 8, column 8). We do not, however, document a positive impact of an increase in the share of capitation on student test scores.

Taken in the context of the existing literature on student learning, the weakness or absence of an effect of greater funding on educational outcomes is not surprising. A simple increase in school resources does not necessarily translate into learning gains for the average student (Glewwe *et al.* 2009; Kuecken and Valfort 2015). Of the 43 high-quality studies (13 of which are randomisations) from the period 1990 to 2010, Glewwe *et al.* (2013) show that most school inputs matter little for improving educational outcomes. Combined with the findings on enrolment outcomes, they are in line with Kremer *et al.* (2013) who conclude, based on a summary of education-oriented RCTs that (i) reducing the costs of school attendance tends to increase enrolment but has little to no impact on learning gains and that

(ii) the provision of resources themselves, unless augmented by additional reforms, does not improve average achievement outcomes.[28]

Finally, we provide tentative evidence that the newspaper campaign allows for a fairer allocation of teachers across schools. More precisely, our analysis reveals that schools closer to a newspaper outlet lose teachers to the benefit of remoter schools after the launch of the newspaper campaign, while those schools closer to a newspaper outlet had more than their fair share of teachers before the campaign. Relying on column 6 of Table 15, we find that a one standard deviation (2.16 km) increase in distance to a newspaper outlet increases the number of teachers by 0.15 standard deviations, hence by roughly one teacher. This pattern may be due to the change in behaviour or departure of the most corrupt district officials.

6. Limitations

A key limitation of our pure replication is the inability to replicate the construction of the capitation grant capture measure in 1995 due to the unavailability of the raw PETS 1996 data. Moreover, it was not as straightforward to investigate the mechanisms behind the capitation grant effect as we originally supposed. A lack of compatible geographic information in alternative datasets prevented us from incorporating other educational indicators (such as the Southern and Eastern African Consortium for Monitoring Educational Quality) as we had outlined in our replication plan. To some extent, this prevented sprawl by limiting our additional analysis to the information that was available in all waves of the PETS: change in the number of teachers as well as change in the other funding sources (teacher salaries and PTA charges). We completed our replication plan before receiving and examining the raw data. Had we waited, we might have been able to outline a more realistic TCA. However, proceeding without the data ensured that we could remain as objective as possible in proposing additional analysis.

Our replication differs from others in that we could not expect to test the robustness of the target paper (RS 2005) without also replicating elements of RS (2011). As described earlier, RS (2005) report preliminary results that are further developed (and, in our opinion, improved) in RS (2011). We thus believed from the beginning that the methods and results of both papers must be considered jointly.

7. Conclusion

Reinikka and Svensson (2005 and 2011) show that a newspaper campaign in Uganda allowed for a reduction in capture of capitation grants for primary education, which, in turn, improved educational outcomes. This study replicates Reinikka and Svensson's results and examines their robustness. Our replication allows us to confirm the overall positive impact of the newspaper campaign. First, its effect of reducing capture is fully robust. Moreover, we do find that the additional funding reaching schools due to the newspaper campaign increased enrolment. Finally, although we cannot support the authors' conclusion that lower capture due to the newspaper campaign improves learning outcomes, an additional positive impact

[28] In a similar vein, it is striking to observe that an increase in the number of teachers has no impact on PLE scores (column 5, Table 11) while it improves enrolment (column 8, Table 8). This result is consistent with Kuecken and Valfort (2015) who show that teacher truancy has no average effect on the reading and mathematics achievement of primary school students in Africa.

of the campaign, overlooked by Reinikka and Svensson, emerges: our results suggest that the newspaper campaign allowed for a fairer allocation of teachers across schools. Our findings therefore call for the continued application of information campaigns in order to stem corruption and improve service delivery in developing countries.

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